memory, for I often in imagination fight again my battles with halibut and skate, lobster and cod, and the words "sea fisheries" have not altogether lost their charm; but I confess I wholly overlooked Prof. Newton's interesting letter on the subject, and turned back the file of NATURE and read the letter in connection with Mr. Holdsworth's. The theory of the former appears to me to be the correct one; the Atlantic sea fishery is being gradually played out.

I doubt whether we can anywhere along its extensive coast now meet with "shoals" of mackerel or "schools" of herring several miles in breadth, forming a mass of so compact a nature that small vessels had almost as much difficulty in getting through them as Sir George Nares had in getting through the so-called palæocrystic ice, or hear of single hauls of 1,000 barrels! And yet such was possible not twenty-five years ago.

The great mischief is done, certainly in America, by trawling. This must be evident if one will but consider the *modus operandi*, by which the female fish are captured just before they have deposited their spawn, a few thousand fish so taken representing the non-existence of many millions. The subject has received the most serious attention of the Canadian Government, and has by it been brought under the notice of the United States and French authorities.

Mr. Holdsworth appears very much amused to find that Prof. Newton has discovered a use for dogfish over and above his own instance, in which it served as "salmon" for the Preston weavers. But as we have never heard that the fishermen of Morecambe Bay were charged with feloniously administering a poison to the said weavers, we must conclude that dogfish is a wholesome, but perhaps not a toothsome, article of food.

Mr. Holdsworth says that with this exception he has never heard of a case in which "the hated dogfish was not knocked on the head and thrown overboard whenever there was a chance of so doing." I can tell him one. Along the American coast the dogfish is certainly knocked on the head, but the fishermen there know its value too well to throw it overboard; they keep it, and it yields an oil; and of the many millions of gallons of "fish-oil" in the returns, "dog-oil" forms no inconsiderable portion.

There can be no doubt that the American fishermen, if they had had their say in creation, would have vetoed dogfish; but as they had not, they came to the conclusion that there was doubtless some wise purpose even in that creature: a thorn in the flesh to try their tempers and their nets, but one which they forced to bear fruit.

If it is true that the nets of the Donegal fishermen in 1875 were constantly full of dogfish, and they driven to their wits' end. I hope some Donegal reader of NATURE will kindly read them this letter; it may be the means of opening up a glorious future for Ireland. Perhaps too some Lancashire reader will give the Morecambe Bay fishermen a hint, in case the Blackburn weaver should hereafter have a surfeit of "salmon," and those practical jokers' occupation be gone.

May I in conclusion be allowed to dissent from Prof. Baird's plural of "alewife?" He calls it "alewives." There is nothing of the meaning of *wife* in the word. This species of herring, which usually goes by the name of *gaspereau*, is also called *alewife*, which is a corruption of the Indian word for a fish, *aloof*. The plural, I think, should be "alewifes." B. G. JENKINS

Dulwich, December 18

Sense of Hearing in Birds and Insects

I do not know whether ornithologists are acquainted with the peculiar manner in which curlews frequently obtain their food on sandy flats which have been left bare by the tide. The birds force their long bills into the wet sand as far as the nostrils, and then again withdraw it, leaving a small hole, which, when probed, is found to be only just large enough to have taken in the bill. The animal, therefore, can only have made a single prolonged push without adding any lateral or exploring movements of the bill, as birds which feed in mud may be observed to do. Now it cannot be supposed that curlews adopt this mode of feeding without obtaining from it some degree of profit. Neither can it be supposed that they make their thrusts into the sand at random; for, their bills being so pointed and slender, the birds would usually require to make a vast number of ineffectual thrusts before they happened to hit upon a worm or other edible object. The question therefore is, How do the birds know the precise spots where their victims lie buried in the sand? That this knowledge is not derived by sight I am quite sure, for I have repeatedly observed innumerable curlew marks of the kind described occurring on tracts of sand which, in virtue of their high level, presented a perfectly smooth and uniform surface. I can therefore only suppose that the birds are guided in their probings by their sense of hearing. Doubtless it is difficult to believe that this sense is so delicate and precise as to enable the curlew to perceive so exceedingly slight a sound as that which must be caused by the movement, say, of a small worm at a distance of ten or twelve inches from the surface of the sand, and at the same time to localise the exact spot beneath the surface from which so slight a sound proceeds. I cannot see, however, that any other explanation is open, and perhaps the one now offered may not seem so incredible if we remember the case of the thrush. No one, I think, can observe this bird feeding and doubt that it finds its worms and grubs almost exclusively by the sense of hearing. And if the distance which it runs between successive pauses for listening represents—as we cannot but suppose it must—the diameter of the circle within which this bird is able to hear the movements of a worm, I think that the hypothesis I have just advanced with regard to the curlew ceases to be improbable.

It seems worth while to add a few words with respect to the sense of hearing in insects. So far as I am aware, the occurrence of such a sense in this class has never been actually proved, although on *à priori* grounds there can scarcely be any doubt concerning the fact of some insects being able to hear; seeing that in so many species stridulation and other sounds are made during the season of courtship. In the case of moths, however, I believe that sounds are never emitted—except, of course, the death's-head. It therefore becomes interesting to observe that an auditory sense is certainly present in these insects. Several kinds of moth have the habit of gently, though very rapidly, vibrating their wings, while they themselves are at rest on a flower or other surface. If, while this vibrating movement of the wings is going on, the observer makes a sudden shrill note with a violin or fife, &c., the vibrating movement immediately ceases, and sometimes the whole body of the insect gives a sudden start. These marked indications of hearing I found invariably to follow a note with a high pitch, but not a note with a low one. GEORGE J. ROMANES

"Towering" of Birds

I HAVE read Mr. Romanes' communication on the "towering" of grouse and partridges with much interest. As he requests further information, may I be permitted to contribute the following:—I once observed a pheasant which, after being shot, flew apparently untouched for about one hundred yards, then towered ten or fifteen yards, and fell dead. As a rule birds that have towered are picked up dead, as Mr. Romanes states; but such is not invariably the case. A correspondence took place in the *Field* some weeks since in answer to the question: "Do towered birds *ever* rise again," and several replies were elicited in the affirmative. The conclusion warranted by that correspondence seemed to be that towering arises from at least two distinct kinds of injury. In the first, the common form, the bird is struck in the *back*, and is always found precisely where marked down. It seems to me that in this kind of towering the *perpendicular* flight may be attributed to a cause perhaps other than, or at all events additional to, *pulmonary* hæmorrhage. I consider that hæmorrhage is a necessary factor, and Mr. Romanes makes out a very strong case in favour of its being into the lungs. That the movements of the wings are convulsive, and the explanation of the towering, I am not inclined to dispute, but I think it has yet to be proved that the convulsive flapping of wings (the directing power of the brain being in abeyance) always produces perpendicular and never merely erratic flight. Every towering bird acts in a precisely similar way. Are we to take it for granted that in asphyxia it is only certain sets of muscles, and these always in the same and to an equal degree, that are spasmodically affected? I have noticed that a towering bird very often has his legs hanging straight down (I do not allude to those cases where they are palpably mutilated), and it strikes me as being likely that paralysis of the legs and lower part of the back may have something to do with the flight being upward. A man who has paraplegia always complains that he cannot move his legs because they are *so heavy*. This sensation would doubtless be felt by a bird paralysed behind, and this, in addition to the loss of its steering apparatus and the co-operating contractions of the posterior muscles, would produce a loss of balance with much the same effect as though the after parts had really become disproportionately heavy. I have no desire to be

dogmatic, but merely offer this as a possible assisting factor in some cases.

In towering from the second kind of injury, the bird can and sometimes does fly away from the place where it fell, and, after retrieving, concordant testimony shows lesion in the neighbourhood of the *eyes*, whence blindness has been assumed to be the cause. The fact that this very rarely occurs, perfectly agrees with the objection that the smallness of the head is adverse to the theory of cerebral injury being the invariable cause. It has further been noticed that these birds seldom move until they are touched. Whether, attention having been drawn to this subject, future observers will detect a difference in the towering of birds that may, and those that cannot, rise again, is hard to say, but I hope, in the interests of science, all pertinent observations will be communicated and admitted into the columns of NATURE.

Faringdon, Berks, December 11 J. HOPKINS WALTERS

The Tasmanians

I SEE it stated in NATURE, vol. xiv. p. 242 (which has just reached us), that M. Castelnau, French Consul at Sydney, states in a letter "to the Geographical Society of Paris, read at its last sitting, that the only four Tasmanians living were presented at the last levée held by the Governor of Tasmania."

I cannot imagine how M. Castelnau can have allowed himself to propagate such an error. It is quite true that four Tasmanian aborigines were presented at a governor's levée, but the presentation occurred just ten years ago, and all the four have long since been gathered to their fathers.

In reference to the last paragraph in your note, I regret to say that we really "have seen the last of them." The sole survivor of this singular race, a female, by name Trucanini, died a few months ago at the age of seventy or thereabouts. The "penultimate" aborigine was King Billy, who preceded Trucanini to the grave three years ago. W. W. SPICEY

Hobart Town, October 21

Algoid Swarm-spores

IN a note on algoid swarm spores, published in NATURE, vol. xv. p. 15, reference is made to the new investigations of M. Sachy, who considers the motion and accumulation of spore as due to currents produced by differences of temperature in the water, and not at all to the action of the light causing the living swarm-spores to move. I do not know the experiments by which this result has been reached; but the following seems to me a confirmation of the new theory.

At a distance of about 5 feet from the window of my room is placed a cylindrical glass vessel of 1 foot in diameter, and containing only some sphagna and microscopical crustacea. This aquarium has been kept unshaken for four years.

Now a great quantity of green alga is collected on the side opposite to the window, while the side turned towards the light is covered with a considerable number of little particles of an amorphous matter, arranged in pretty regular cloudy forms, containing nothing but *débris* of plants or animals, and a few desmidiæ.

These particles, which cannot be considered as *living matter*, arise from the light mud which covers the sand at the bottom of the aquarium. The right and left sides of the vessel remain quite clean.

I should much like to know if any of your readers have observed similar facts. E. RODIER

29, rue Saubat, Bordeaux, December 17

Meteor

I SAW the meteor spoken of by your correspondent (*ante*, p. 170) at Blackwater on Wednesday the 13th inst. at 4.45 P.M. as I was passing down St. James's Square. It was apparently of somewhat greater magnitude than the planet Jupiter, and passed from north to south, till it disappeared behind the houses.

Your correspondent will find two notices of the same meteor in the *Times* of the 15th inst. P. L. SCLATER

ON THE RELATION BETWEEN FLOWERS AND INSECTS[1]

THE habit possessed by our honey-bee of feeding itself from flowers, and its corresponding faculty of deciding amongst different species and divining the situation of the honey, is, in the first instance, derived from the common parents of all the Hymenoptera. It probably even comes from such remote ancestors as the leaf-cutting wasp, from them passes to the gall-flies, the ichneumons, and the hunting-wasps, from which latter it goes to the allied species of ants and bees. We may see all these families of Hymenoptera feeding on the honey and pollen of flowers, and manifesting a certain, if not always very obvious, intelligence in choosing the flower to be visited.

The various families of wasps differ amongst each other as to their ingenuity in finding the honey, but it is in the bees that we first arrive at the more complex use of the food, *i.e.*, not merely for the insect itself, but also for its young, combined with such intelligence in its discovery, as proves that the most highly developed form of insect is the one which profits by the honey lying most concealed. The following observations may throw some light on the foregoing statements:—

I come to the conclusion that the Hymenoptera enumerated have a certain degree of intelligence, at least with regard to honey that is in sight, from never having seen leaf-cutting wasps or ichneumons, and still less hunting-wasps or bees, seek honey so long in flowers where it does not exist as is the case with some species of beetles, which feed frequently or exclusively on the nourishment derived from flowers.

However, even very highly-organised insects are at times misled, and Dr. Müller cites one case in which *Melampyrum arvense* was surrounded by a crowd of ichneumons, bees, &c., seeking the honey in vain, the only one which succeeded in obtaining it being *Bombus hortorum*, which has the longest proboscis of all our humble-bees.

It cannot be said either of the leaf-cutting wasps or of the gall-flies that they attain a high degree of intelligence in finding concealed honey, and to these we may add the ichneumons which are frequently found on plants with the honey easily seen (Umbelliferæ, Listera, Ruta, &c.), much more rarely on those where it is partially concealed (Cruciferæ, Spiræa, Salix), and quite as an exception on those in which it is completely hidden (Gypsophila, Malva, Mentha.)

When once a family of Hymenoptera has attained to the point of intelligence of providing food for its young and placing it along with the eggs, we see it develop greater dexterity in its search for honey. In comparing, for instance, the statistics of the visits of the leaf-cutting wasp and the hunting-wasp, we find that even the most developed leaf-cutting wasp only attempts to rob those flowers whose simple form renders the honey easy of access. Even those of Bryonia and Reseda seem unattainable by them. On the contrary, we see the hunting-wasps attack not only these, but also flowers specially adapted to the movements of the fossorial Hymenoptera, for example, Echium, the Labiates, and the Papilionaceæ, and also the pendent bells of Symphoricarpus, which only allow ingress to the honey from below. It must be deduced from the above statements that flowers and the insects which visit them are adapted to each other, and have gone through corresponding degrees of development at each period of the world's history. For example, if my view of the origin of the Hymenoptera is correct, there has been a time when species with an ovipositor were the only Hymenoptera; and when only regular, open, turned-up flowers of as low a form as Salix existed, while Reseda, Echium, the Labiates, the Papilionaceæ, &c., &c., have been developed at a later period after the species of Hymenoptera had developed to the point of preparing a place for their young.

We may therefore see how through the transition of hunting-wasps to the habits of bees, and further within the bee-like family, dexterity in acquiring the food has increased. The species perhaps most nearly allied to the ancestors of the bee—Prosopis—is, as to its organisation,

[1] Abstract of an article in the *Bienen Zeitung* by Dr. H. Müller.